NEW YORK NEUROLOGICAL SOCIETY.

Stated Meeting, November 7, 1882.

A stated meeting of the Society was held at the usual hour and place, Dr. E. C. SPITZKA in the chair. Drs. M. Josiah Roberts and G. W. Jacoby were elected to active membership.

Before proceeding to the paper of the evening, Dr. Spitzka read some notes sustaining his priority to the observation that certain artificial precipitates are formed in nervous tissue in preparations by alcohol, and criticising Dr. Dana's report upon the post-mortem appearances presented by Guiteau's brain.

Dr. Spitzka said: As early as February 5, 1877, at a meeting of this Society, in a paper entitled the "Psychological Pathology of General Paresis," I referred to the production by alcohol of artificial precipitates in brain tissue. I further stated that artefacts thus resulting had been described as lesions of paretic dementia.[1] In a second paper, read in March of the year following, entitled "Reform in Scientific Psychiatry,"[2] I based criticisms of the work done at an asylum in this State on the allegation that such artefacts constituted its basis; and in a sort of sequel[3] of this latter paper, read October 5th of the same year, I challenged the American Association of Asylum Superintendents, offering to produce at will the lesions on which the so-called results of the Utica and Oshkosh laboratories were based, in the brains of men or animals previously ascertained to be healthy. That challenge, most of you will recollect, was not accepted, nor was it noticed. Subsequently, I gave a more detailed account of the nature of the precipitates in a paper[1] published two years ago.

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[1] JOURNAL OF NERVOUS AND MENTAL DISEASE, April, 1877.

[2] JOURNAL OF NERVOUS AND MENTAL DISEASE, April, 1878.

[3] Merits, Motives, and Prospects of the Movement for Asylum Reform. JOURNAL OF NERVOUS AND MENTAL DISEASE, October, 1879.

I have been thus explicit in referring to these old publications, because it has unfortunately become necessary for me to assert my claim to priority in observation, which cannot, I think, be disputed. It is perhaps not so remarkable that Dr. Savage, one of the editors of the *Journal of Mental Science*, should read a paper before the International Medical Congress, in the summer of 1881, in whose abstract I fail to find any notice of the previous discovery and description by an American investigator, as it is surprising that a publication should emanate from a member of this Society dealing with these bodies, and ignoring the fact that they had been previously abundantly noticed.

In my latest paper I stated substantially the following:

"When the brain or cord of any animal is submitted to the action of absolute alcohol a sufficient length of time, a peculiar series of changes occurs, simulating lesions, but which can be produced at will in the healthiest human and animal brains. In the first place, alcohol has been long known to extract *leucin* from nervous tissues, and I have found crystalloid or subcrystalline spheres of *leucin* scattered more or less regularly through the tissues, or accumulated in the perivascular areas of healthy brains submitted to the action of alcohol. By varying the length of exposure, or by immersing the brain, or different portions of the same brain, at different stages of post-mortem change, in alcohol, these artificial 'lesions' may be pleasingly varied. That these bodies are *leucin*, or at least chiefly consist of *leucin*, is demonstrated by their chemical reaction. Among the tests which can be conveniently applied, are their insolubility in ether and chloroform, and difficult solubility in alcohol (requiring 1,040 parts of cold and 800 parts of hot alcohol); the bodies do not stain in carmine, and are refractive;[2] besides, they are either hyaline or show a radiatory striation.[3]

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[1] What Has Been Done by the Asylum Association, etc. *Chicago Medical Review*, June 20, 1880.

[2] This feature varies, probably according to certain admixtures.

[3] Overstaining sections containing these bodies in carmine, and particularly in hematoxylin, does stain the latter; ordinary staining fails to do so.

"As I have stated, variations in the external conditions may possibly have something to do with variations in the composition and appearance of such bodies.

"Now the observer who has not had his attention called to these precipitates before, and who, finding them in the brain of some subject dying from a degenerative brain affection, or from some disorder like hydrophobia or delirium, symptomatic manifestations presumably associated with intense molecular and bio-chemical transformations, may be excused for jumping at the conclusion that these bodies represent *bona fide* lesions.

"But when, as in the asylum under consideration, the same 'change' is found in such different affections as general paresis, subacute mania, and chronic melancholia (cases 2, 3, and 5 of the text), it becomes evident that as little reflection has been exercised as when *destruction* of nerve tracts is considered to be the basis of hallucinations.

"In the text the writer of the paper names them 'granular bodies,' (p.10); '*grumous* granular matter,' (p. 17); 'encysted morbid products,' (p. 21); 'colloid bodies,' (p. 23); globules 'of a fatty nature,' (p. 27); and the fact that he has found such deposits in such different forms of insanity, has evidently furnished the grounds for the following statement:

"'A fact which seems of the utmost importance is the similarity of histological changes attending the different forms of insanity, as represented in the micro-photographs, and indeed in all cases which have fallen under observation. If such regularity is displayed in future investigations as I am strongly led to believe will be the case, this fact will practically confirm the principle, that in insanity we have to contend with *only one* DIATHESIS, manifesting itself under different phases in its progress and results.'

"The reflection forces itself on the reader, that if the artificial lesions were manufactured to order, as it were, in every other class of diseases, that it would be an equally just inference to refer all somatic affections to only 'one diathesis.'"

Referring to a paper by Kempster in the Wisconsin State Medical Society Transactions, 1877, pp. 43-58, I wrote:

"On page 51 is an excellent description of the artificial precipitates:

"'Just how these spots originate is not yet well settled. They have been supposed to originate in the nuclei of the neuroglia or nerve glue, which fastens the nerve fibres together, and which consists of a delicate structure made up of cellular material but compact and strong. I am not prepared to state that they do not originate there, but from certain specimens in my possession and which I will show you, it will at once occur to you that the white substance of the brain must be principally made up of nuclei of neuroglia, and that there is little room for any thing else, as the field of the microscope is completely covered by these small masses. They are from the 1-500 to the 1-4000 of an inch in diameter, are clear, of a grayish-white color, and have no perceptible centre or nucleus; the margins are distinct, and they maintain their individual relations notwithstanding the fact that they are sometimes found closely pressed together. In these minute growths I have failed to find any evidence of any known tissue, notwithstanding that they have been subjected to a prolonged and critical micro-chemical manipulation.'

"Others, which I have repeatedly produced in the brain of a healthy dog, are described as 'miliary sclerosis,' regarding which he gives the following, more than remarkable, explanation:

"'Another peculiar feature observed, which I will allude to here, but speak of more fully hereafter, that is, that these large masses are found in tracts of nerve tissue previously affected with true sclerosis; and when they are found in sclerosed tissue the spot is not fibrous. When found in tissue not sclerosed they are fibrous. * * * * It is not unusual to find crystals lying upon the surface of the spot, but these are not at all characteristic and found in the normal brain tissue after treatment with alcohol.'

"One step farther, and a little more of judicious comparison, and the writer would have discovered the true nature of the 'lesion.'

"He has the excuse of a precedent for his error, however.

Some of the 'lesions' described can be produced in the brain cortex after an examination of the tissues in the recent state has shown it to be healthy, and it can be produced by using alcohol on one specimen, while specimens from the same convolution hardened in bichromate of potash, or other reagents than alcohol, fail to show it. What Dr. Kempster describes at the bottom of page 43, is the earlier stage of the same artificial precipitates which, in the middle of the same page and the first paragraph of the next, he describes as lesions of a different kind."

The main significance of these descriptions to-day consists in the fact that the medical officers of both the Utica and Oshkosh asylums swore not only that Guiteau was not insane, but also that he was shamming. They further swore at his trial that the microscope always demonstrated visible brain disease in insanity. Indeed, they had both previously committed themselves to such a view in print, contradicting every real authority; the superintendent of the latter asylum doing so in the following emphatic words: "Even in the most recent cases of insanity, although the change may not at once be detected by the unaided eye, the microscope reveals the change and unerringly points out the seat of disease."

Take in connection with this, the fact, which will be made evident from the specimens to-night, that the same bodies photographed at both asylums as the essential lesions of insanity, the same bodies on which Dr. J. P. Gray bases his theory that they represent the basis of hallucinations and delusions, were found in the brain of Guiteau!

It is scarcely necessary for me to say that if any member of the New York Neurological Society were in the predicament of either of these gentlemen to-day, he would either —if sincere in his belief that these bodies were *bonâ fide* lesions—hasten to admit that he was wrong in pronouncing Guiteau sane; or—if shaken in that belief—confess that his statements as to the constancy of pathological findings in the insane, and the genuineness of the body referred to, were incorrect or premature.

But even members of this Society may become the subjects of a legitimate criticism in connection with the report of the microscopic examination *in re* Guiteau. During the summer Dr. William J. Morton allowed me to examine a series of sections obtained from Guiteau's brain, and it was but a few minutes before we discovered a pyramidal nerve-cell of the cortex presenting in a marked degree the evidences of beginning fatty granular change; slighter evidences of the same disintegration were found in other cells. On the same occasion I called Dr. Morton's attention to the artificial precipitates in the section, which had been noted by the preparers and examiners, without any decided opinion as to their character having been arrived at. If my recollection serves me right, Dr. Morton recognized the identity between these bodies and those described by myself and just adverted to, of which I showed him specimens at the time. What was my surprise on reading a "report" signed by Drs. C. L. Dana and Wm. J. Morton, to find that these bodies were referred to as the result of post-mortem decomposition, not the slighest notice being taken of my earlier and repeated description of these bodies in 1877, 1878, 1880, and at other times before this Society. It is but fair in considering this report to say that Dr. Morton had no knowledge of its existence until I called his attention to it; and that his name was affixed to the report under a misapprehension, and without his authority or knowledge.

As this report stands, it incorporates an erroneous view as to the nature of these bodies, inasmuch as its writer regards it as an open question whether they may not be the result of post-mortem decomposition, whereas they are really due to the imperfect methods of preparation resorted to. The general conclusion is that nothing of note in the way of a morbid appearance, such as is found in insanity, could be discovered in the sections. Through the kindness of Dr. Morton I was enabled to examine representative sections of all the specimens in the hands of the "committee," and owe to him the set of duplicates herewith exhibited. I find, in contradiction to the report, that notwithstanding the tissues are improperly preserved, indifferently hardened, so badly cut (in consequence), and so poorly stained, that a positive statement that the actual tissues were normal would in itself appear suspicious, that there is not a healthy section among them. I have marked a few spots for reference, and should be very happy to exhibit them to any gentleman interested, or to any committee that the Society would name, in daylight; I regard our available illumination to-night inadequate for finer study. It is remarkable to what extent the discoverable lesions in the specimens constituting the basis of Dr. Dana's report, correspond to the description furnished by the officially authorized committee, whose report was drawn up by Dr. Shakespeare of Philadelphia. The vascular as well as the cellular changes described in the " Shakespeare report " can be discovered—not in isolated examples, but in profusion—everywhere throughout the sections obtained from Dr. Morton.

I am also able to exhibit to the Society this evening, through the courtesy of Dr. Shakespeare, a specimen from the frontal lobe and one form the corpus striatum of Guiteau. These sections were prepared by Dr. J. C. McConnell of the Army Medical Museum: a glance will show that they are beautifully cut; the staining might be a little deeper, or possibly the method of preservation may have prevented the staining of the ganglionic bodies as one might wish to see them stained. The atrophy of the outer cortical layer is evident in the section, though it is difficult to say how much this appearance may be intensified by a cellular infiltration of that zone. The lesions of the blood-vessels are distinct and intense; one of the long arterioles particularly shows the kinking and corrugation, as well as the residue of old and recent hemorrhages in its adventitia, common with the chronic insane, in a most typical manner.

It was not my intention to-night to dilate on the signification of these appearances, or their bearing on the individual case, in which it was my fate—without any provocative act on my part—to become a confessedly most interested party. Nor do I intend that this communication shall elicit a discussion of the general questions of the trial, of the mental state of Guiteau; or even of the hypothetical bearing of such lesions as those represented in the exhibited specimens, on mental symptomatology. I desire simply to present facts and criticisms which may be tested to-day and to-morrow on the basis of facts; and I have selected a Society, and naturally this one, as a medium of communication, because I could thus best secure a fair hearing and a full re-criticism in the presence of facts.

At the conclusion of Dr. Spitzka's remarks, Dr. MORTON, who occupied the chair *pro tem.*, said that it would be better that he should be the first to speak upon this subject, since evidently a misapprehension in regard to his signing the report in the *Medical Record* had arisen.

It was natural, from several points of view, that Dr. Dana should have supposed that his (Dr. Morton's) views coincided with those expressed in the report, since he had gone to Washington with Dr. Dana, had reported upon the superficial appearances of Guiteau's brain together with him, and had had sections of this brain prepared by Dr. Porter, in Dr. Satterthwaite's laboratory, in which Dr. Dana was working. He had also glanced over the specimens hurriedly, being on the point of departure for the country for the summer; and though pointing out to Dr. Dana certain appearances that he held to be probable artificial precipitates, and other evidences of a morbid condition of certain cells, he had, nevertheless, expressed no decided opinion upon the subject, but left it entirely open. A few hours subsequently, in conferring with Dr. Spitzka, he felt confirmed in these views, but reserved any expression of opinion until he had time to make a thorough examination. Dr. Dana, not fully aware of his position, and supposing it to be similar to his own, had appended his name to the report, a fact of which he was ignorant, owing to the miscarriage of a letter from Dr. Dana.

Dr. DANA remarked that Dr. Morton had correctly stated the situation, and the mistake had probably arisen from the loss in the mail of the proof-sheets that had been sent to Dr. Morton. He regretted that he had not previously seen an account of Dr. Spitzka's prior description of the artificial precipitates referred to, but suggested that their publication had not been sufficiently widespread to attract attention.

Dr. MORTON further stated that his position in regard to the histological post-mortem changes found in Guiteau's brain was fully expressed by the "Shakespeare report," as was evident from the fact that he had published this report in full in the JOURNAL OF NERVOUS AND MENTAL DISEASE with editorial sanction.

Dr. SPITZKA, in concluding the discussion, remarked that the best pathologist would have been unable to make a satisfactory examination of the specimens from which Dr. Dana derived his sections, while a less able pathologist would have been in a position to make a good examination of those prepared by Dr. McConnell. He had been under the impression that the specimens had been prepared by Dr. Satterthwaite's assistants or at his laboratory, because to the best of his recollection, the laboratory and Dr. Satterthwaite's assistants were accorded prominent mention in Dr. Dana's report.

Here Dr. DANA interpolated that that was a mistake, that he had himself prepared the specimens upon which the report was based.

Dr. SPITZKA continued, saying, that with regard to Dr. Dana's omission of priority-acknowledgment, that the very paper referred to by Dr. Dana as the only one found by him was published much less prominently in the same medium with those referred to. His description there quoted of colloid transudation and vascular stasis, referred to a genuine lesion in paretic dementia, and not to artefacts. He thought, while not questioning Dr. Dana's intention of giving due credit, that it was a little hard, in view of the common assertion as to the scarcity of original scientific work in America, that when such work was done, it should remain unknown not only to a gentleman across the water, who, like Dr. Savage, was the editor of a journal that had reviewed the first of his papers, but also to those residing in the same community. He added that he would have been less critical on this and other heads, if the fact had not been so strongly apparent to him, that the report referred to would be looked upon as one—however forced—confirmation of the position taken by the *N. Y. Medical Record* in its editorial expressions on the Guiteau question.

The paper of the evening, entitled "The seat of instinct," was then read by its author, Dr. WM. A. HAMMOND.

Dr. Hammond held that the seat of instinct was in the medulla oblongata, or in the spinal cord itself.

Numerous instances were brought forward to sustain this proposition. Frogs whose cerebral hemispheres had been removed would execute the swimming movements perfectly. If the medulla were removed, leaving only the cord, they executed no movements of the kind—hence the seat of this instinct, so strong in the frog, was in the medulla.

Maine de Biran had recorded an instance where the head of a snake had been cut off and yet it found its way with great certainty to its customary hole in the wall. Again, anencephalous monsters sucked, cried, and opened and shut their eyes. From a large array of facts of this nature, Dr. Hammond felt justified in concluding that instinct resided in the medulla and cord.

The discussion was opened by Dr. PARSONS who said, that while he could agree with some of the points advanced by Dr. Hammond, he could not unreservedly agree with him in others.

He thought the question regarding the seat of instinct could be best investigated by studying its manifestations in the lower order of animals and plants, and by and by considering instinct in its broad rather than in its restricted sense; that is, by proceeding from the simple to the complex, rather than by commencing with the complex.

He applied the term instinct to all involuntary acts or movements of the whole, or of parts, of living beings which were produced by the direct agency of the being, whether within or without the sphere of consciousness, and whether excited by external forces or not. In its broad acceptation they term instinct reflex and automatic acts of every kind and degree. This meaning was in accordance with the design of the instinctive powers, which was the preservation of the individual and the continuation of the species.

Hence, the innate force by which the protophyte or protozoön gets its food, or resists injurious influences, was as really instinctive as the more complex and apparently intelligent acts of higher beings, such as the constructive instincts of bees and birds, the instincts which lead animals to seek proper food, or avoid their enemies, or the involuntary movements of the body which are caused by the emotions.

But the protophyte or protozoön had an organization so simple that each part performed the same functions as every other part. In some of the lower orders of beings reproduction was accomplished by a process of scission, the being dividing itself into two parts, each of which was as complete as was the original whole. There was no differentiation of organ and function, and no nervous system in these lower orders, each and every part performing such vital acts as are required for preservation or reproduction. In these beings the instinct had its seat in general in every part, and in particular in whatever part the force was exerted. Higher in the scale of organization special organs began to appear, or were differentiated from the homogeneous mass, for the performance of special functions, as the digestion of food. The stomach, for instance, then performed a specific vital act which was innate or instinctive in character. The force had its seat in the organ that performed the act.

Still higher in the scale of organization, nerve-masses connected with the stomach, and to a greater or less degree controlling its functions, were differentiated. Here the seat of the instinctive force was in part in the nerve-mass, and in part in the stomach itself, the force in each being responsive to that in the other.

Yet higher in the order of development certain innate or instinctive impulses came within the sphere of consciousness. The mind cognized that certain instinctive acts were beng performed, and to a greater or less extent controlled or regulated their performance, while at the same time the act itself was quite involuntary, or might even be in opposition to the volitions. Many bodily acts of an instinctive character, such as blushing, increased or diminished action of the heart, and the flow of tears, were often caused by the emotions; or, in other words, by the agency of the higher nerve centres within the brain.

A particularly good illustration of the various phases of instinct, and of the changes that take place in its seat, might be found in the reproductive or sexual instincts. In man, for example, the sexual instinct did not exist during the fœtal state and the early years of infancy. At a certain stage of development the reproductive organs began to evolve their force. Hitherto it had not been manifested in the higher nerve centres. But as soon as the ovaries or testes began to develop their reproductive functions, the higher centres, which hitherto had possessed the sexual instinct only in potentiality, began to exercise the function, or to develop the force, in reality. The reproductive instinct had its origin in the reproductive organs, and these organs never lost the force they originated. But as soon as the responsive or coördinate instinct was developed in the higher centres, these higher centres also became originators of the force. The reproductive organs stimulated the emotions; and the emotions themselves, aroused through the senses, stimulated the reproductive organs. The sexual instinct had its seat not alone in the spinal cord, in the brain, or in the reproductive organs, but in each of the three.

Dr. Parsons considered instinct to be not one, but many; so that the answer to the inquiry regarding the seat of instinct might be best found out by ascertaining where in the organism this, that, and the other instinctive force had its seat; and if we found out, as seemed to be the case, that one or other of the instinctive forces had its seat in every tissue of the body that performs a vital action, we must arrive at the conclusion that instinct has its seat in all parts of the body, each particular instinct having its seat in the part or parts which exert the instinctive force.

He thought the question regarding the seat of instinct was analogous to the question regarding the seat of the retentive power, or the memory. There was no especial organ of the memory, but memory, in a physiological sense, was a quality or condition of an organ or tissue. A certain part of the brain, for instance, cognized the relation between ideas and the expression of ideas. Having cognized certain modes of expression, as words, for example, it retained the impression it had received. It remembered words. Another portion of the brain in like manner cognized and remembered forms. Another portion directed the fingers, for instance, to do certain acts again and again. It retained or remembered these acts. But after many repetitions the eccentric nerve tissues immediately concerned in these movements acquired and retained the ability to produce them of themselves, without the influence of the central originating force ; or, in other words, eccentric nerve-tissues had the faculty of memory.

Dr. LEONARD WEBER thought that the brain could not be excluded from participation in the acts due to instinct. Dr. Hammond's location of the seat of the instincts was too narrow—such experiments and observations as had been brought forward were not, in his opinion, sufficient to sustain the author's position.

Dr. GRAY asked the reader of the paper for a definition of instinct.

Dr. HAMMOND thought that instincts had no relation to perceptions.

At this stage in the debate, a motion was carried to suspend the immediate business of the evening in order to allow Dr. MCBRIDE to read resolutions of sympathy relating to the affliction that had fallen upon a most valued member.

The resolutions were as follows: *Whereas*, the greatest calamity that can befall man—the sudden destruction of all that which makes life dear—has overtaken Dr. E. C. Seguin, an active member of this Society, and the first President after its reorganzation ;

Resolved, That this Society expresses to Dr. Seguin at this time of his great grief and sorrow, its warmest and heartiest sympathies :

Resolved, That while appeciating how painful it would be to resume work in the community in which such a sad bereavement has occurred, nevertheless it is the earnest hope of the Society that Dr. Seguin will resume and soon return to his scientific and professional labors. As a teacher, investigator, and practitioner he has a place which cannot be easily filled.

Resolved, That an engrossed copy of these resolutions be transmitted to Dr. Seguin by the Society.

On motion of Dr. HAMMOND these resolutions were unanimously adopted.

The discussion was now resumed, Dr. GRAY taking the ground that instinct could have no special location, but was to be found rather in varying degrees in every portion of the gray matter of the central nervous system.

Dr. PUTZEL contended that the instinctive acts were those that were due to an inheritance in the individual derived from ancestors in whom similar acts had once been volitional and deliberative.

Dr. SPITZKA asked for further information in regard to the anencephalous child referred to by Dr. Hammond as being able to suck, and open and shut its eyes.

Dr. HAMMOND related the case in full as reported by St. Hilaire. From this report it appeared that all the nervous parts above the foramen magnum were absent.

Dr. SPITZKA could not see how an act of the nature described could take place without the existence of the cranial nerves involved in the act.

Dr. HAMMOND was willing to accept the fact as reported on the authority of an observer as competent as St. Hilaire.

Dr. DANA agreed with Dr. Putzel that instinctive acts were the result of previous ancestral experiences. For instance, the fly-catcher just out of its shell, would capture a passing insect with almost unerring accuracy. This act was undoubtedly an organic capacity derived from a store of previously registered impressions.

Dr. SPITZKA regarded instinct as an inherited tendency residing in species, and confined mainly to acts, whose purpose was to propagate and preserve the individual.

He refered to certain tendencies in the human race which resembled the instincts in the lower animals. A good example of this was to be found in the case of the idiot girl who, delivered of a child, proceeded to gnaw through the umbilical cord. As an instance of the modification of instinct, he cited the instance of the Scandinavian mares, which to this day, after foaling, eat the placenta, while the mares in England, from whom, during long ages, the placenta has been at once taken away, exhibit no tendency to devour the after-birth. Dr. Spitzka referred to the "Triebe" of the Germans as being analogous to the instincts.

Dr. PUTZEL coincided with these views.

Dr. HAMMOND regretted that the discussion had drifted from the subject. The question was not as to the nature but as to the seat of instinct.

He maintained that his position was not to be invalidated by simply denying the accuracy of the observations that he had brought forward.

The idea of his paper was to prove that certain acts generally accepted as instinctive, existed if the medulla oblongata remained uninjured. He had therefore located the seat of the instinct in this organ.

The Society then adjourned.

Stated Meeting, December 5, 1882.

Before the reading of the regular paper of the evening, Dr. SPITZKA made a brief verbal statement concerning the case of an infant surviving four months after removal of a large portion of the brain.

Dr. LEONARD WEBER then read a short paper, entitled, "Case illustrating the causal relationship between locomotor ataxia and cardiac disease."

Certain facts had been reported showing a causal relationship between locomotor ataxia and certain cardiac lesions, by Grasset in June, 1880, and by Drs. Berger and Rosenbach, of Breslau in 1879; but in the works of standard authors on neuro-pathology, the reader had not found observations bearing on the subject in question.

His patient, æt. forty-five, had presented during the last four years nearly all the classical symptoms of posterior sclerosis. In Aug., 1880, attacks of orthopnœa developed in association with violent cough, expectoration of frothy mucus, râles, pericardial *frémissement*, and friction sounds. The systolic sound was muffled at the apex. A diagnosis of pericarditis and endocarditis was made. The attack had come on immediately after the sexual act. A number of attacks of a similar nature occurred from time to time, always after the sexual act. His excessive indulgence probably furnished the immediate cause of the developement of the cardiac disease, while the remote cause was to be sought in the spinal affection, and its weakening influence upon the circulatory system, as explained by Charcot and Eulenberg.

After a brief discussion the Society adjourned.